mine under normal conditions; they were taken in a sealed-off portion of Seaham Colliery after an explosion and an underground fire, and thus represent what took place under an entirely abnormal condition of the mine. Apart from this point, the Author of the Warnings contrives to give his reader the impression that Mr. Corbett's Seaham Colliery records entirely favour his own views, that high barometric pressure causes an increase in firedamp in mines; so far from this being the case, however, Mr. Corbett's own words (Trans. North Eng. Inst. Min. Eng., vol. xxxii., 1882–3, p. 310) are:—"It is well known that gas is frequently found in colliery workings before any fall of the barometer commences. . . . The barometer, so far as an indication showing that gas may be expected, cannot be said to be reliable." In the discussion of this paper, Mr. J. Daglish (*ibid.*, p. 311) said that he had made experiments at Hetton Colliery, and that "the results he arrived at were precisely such as were given by Mr. Corbett, namely, that there was no connection whatever between the variations of the barometer and the prevalence of gas in the galleries of the mine." The chief witness cited in his favour by the Author of the Warnings is thus seen to give evidence quite directly against him when he is quoted correctly. Further, if the Author of the Warnings attaches the importance that he appears to do to these records of pressure, why does he not quote the very well known and much more applicable experiments of Sir Lindsay Wood, who determined the pressure of firedamp in normal coal seams by boring holes into them and inserting pressure gauges? His general conclusions (Trans. North Eng. Inst. Min. Eng., vol. xxx., 1880–1, p. 224) are:—"There is no connection between the variations of the barometrical column and the temperature with the quantities of gas evolved"; only in one set of tests, namely, at Eppleton Colliery, was any connection traceable, and, respecting these, Sir Lindsay Wood (*ibid.*, p. 182) states:—"With the barometer steadily rising, the gas pressure (with one or two exceptions, when there was an increase) steadily decreased."

Personally, I attach relatively little importance to records of pressure alone, even to such careful ones as those of Sir Lindsay Wood, Nasse, Broockmann, &c.; in the absence of analyses, it is only a conjecture that the pressure was caused by firedamp, and in the case of Seaham Colliery it is quite likely that other gases were present in large quantity. I hold that there is only one correct method of attacking this question, as has already been pointed out by Oberbergrat G. Köhler, and that is by systematic chemical analyses of the return mine air combined with barometric observations, as has been done on several occasions on the Continent, *e.g.* by Hilt at the Gemeinschaft and Alt-Gourley pits at Aachen, and, above all, by W. Köhler at the Grand Duke Frederick pits at Karwin. All the observations corroborated each other, and agree with the summary of W. Köhler:—"The proportion of firedamp in the air of the mine decreases in general with rising atmospheric pressure, and increases with falling atmospheric pressure. The proportion of firedamp increases the more rapidly the more steeply the curve of atmospheric pressure descends, and decreases the faster the more steeply the curve of atmospheric pressure rises." Harzé in Belgium and Behrens in Westphalia have confirmed these conclusions in their elaborate works on the subject. All this is the result of accurately observed facts, into none of which "theory" enters. All workers and observers in this subject have come to one of two conclusions, either that barometric variations have no decisive influence on the evolution of gas, or else that a falling barometric gradient increases the outflow of gas. Not a single writer, so far as I know, shows that a rising barometer increases the evolution of firedamp. Whilst most English authorities hold the first view, the universally held opinion in Germany is summed up thus by the well-known Saxon authority, E. Treptow:—"Im besonderen ist es als erwiesen anzusehen, dass nach einem schnellen Fallen des Barometers stärkere Gasentwickelungen stattfinden. Es ist daher die fortlaufende Beobachtung der Barometerstände von grosser Wichtigkeit; tritt ein Barometersturz ein, so ist besondere Vorsicht geboten. Ein Barometerfall von 1 mm. in einer Stunde ist schon sehr bedeutend." (In particular, it may be looked upon as demonstrated that, after a rapid fall of the barometer, stronger evolutions of gas take place. The continual observation of the height of the barometer is therefore of great importance; if a drop of the barometer takes place, special caution must be observed. A fall of the barometer of 1 mm. per hour is already very serious.)

It is facts like the above-quoted analyses that alone can decide this question; it is quite useless to inquire whether the barometer was high or low at the time of any particular colliery explosion, because a serious colliery explosion can only be brought about by the fortuitous coincidence of a number of contributory conditions, only one of which (and in all probability a relatively unimportant one) can be ascribed to the state of the barometer. The Author of the Warnings implies that my views have been influenced by newspaper statements as to the height of the barometer at the time of the great Courrières disaster; but not only do I, as I have said, regard such evidence as useless, but, above all, I would not commit the crowning absurdity of quoting in a discussion on firedamp the Courrières explosion, which is perfectly well known to have been a coal-dust explosion in a non-fiery pit.

Perhaps the most interesting point in the letter of the Author of the Warnings is his explanation of the reason why high barometric pressure must increase the percentage of gas in a pit; he believes that the increased pressure of the air squeezes down the earth's crust, and squeezes the gas out of it. I presume that he wishes this explanation to be taken seriously; but surely he has overlooked the very obvious fact that any increase of pressure on the surface of the earth, tending to squeeze gas out, is counterbalanced by an exactly equal increase of pressure upon the face of the coal in the mine, tending to keep the gas in, and that no variation of atmospheric pressure can thus disturb the previously existing *régime*. Even if this were not so, and if the crust of the earth could respond to such pressures, they are too insignificant to have any practical effect. An enormous fluctuation of barometric pressure, such as a rise of 1 inch, would correspond to a pressure on the earth's crust of only 70 lb. per square foot, or a good deal less than that of an ordinary crowd of people standing on the ground; the very suggestion that such a trifling weight can have any effect through thousands, or even hundreds, of feet of strata is so absurd as to require no refutation, and least of all to the mining engineer who has had to timber underground workings, and who knows that the roof pressure in a mine must be gauged, not in pounds, but in tons on the square foot, and that 70 lb. more or less will make no practical difference whatever. That such a theory should be relied on in defence of the Colliery Warnings surely justifies their opposition by mining engineers, and forms an emphatic endorsement of the verdict of the last Royal Commission —which, by the way, was not composed of professors of mining or theorists—upon these Warnings as *misleading and serving no useful purpose.*

H. LOUIS.

The Afterglow of Electric Discharge in Nitrogen.

IN a paper published in the current number of the Physical Society's Proceedings, I showed that the yellow afterglow produced by the electric discharge in rarefied air is due to the oxidation of nitric oxide by ozone, both substances being formed in the discharge. In a second paper, in course of publication, it is shown that several other oxidisable gases or vapours inflame spontaneously when mixed with ozone at a low pressure, and burn with phosphorescent flames of low temperature.

An afterglow in nitrogen has been recorded by Mr. Perceval Lewis (*Phys. Zeit.*, v., p. 546, 1904) which is obtained only with condenser discharges. This glow is orange in colour, and possesses a visual spectrum of three bright bands in the green, yellow, and red regions, in contrast to the continuous spectrum of the glow which I have traced to nitric oxide and ozone.

I have recently experimented with Lewis's nitrogen glow, using the method, introduced by Dewar in 1888, of drawing a continuous current of the gas through the vacuum tube into another vessel on its way to the pump.

I succeeded at once in obtaining it, when the condensed discharge was used. This glow has many interesting properties, of which a preliminary publication seems desirable.

I believe it to be due to pure nitrogen. Lewis states that it cannot be obtained from atmospheric nitrogen, but this does not agree with my experience. I have used atmospheric nitrogen exclusively.

The glowing nitrogen is unaffected by silver gauze, which quenches the ozone glows. It is destroyed by mixing oxygen with it, but merely diluted by hydrogen or ordinary nitrogen. When acetylene is led in, a bright flame is produced at the point of confluence. This flame replaces the original glow. It has a spectrum consisting of the swan and cyanogen bands, along with others not identified. If the nitrogen glow is led over iodine a magnificent blue flame is produced, contrasting sharply with the original orange glow. With sulphur the original orange glow is quenched, but no other replaces it. The sulphur becomes hot, and a metallic-looking sublimate is formed along the tube.

The most remarkable phenomena, however, are with metallic vapours, which give line spectra when the glowing nitrogen is led over them. Sodium, potassium, thallium, mercury, zinc, cadmium, and magnesium have all yielded line spectra in this way.

Investigation is being pushed on as fast as possible, but the facts so far obtained seem to point to the production of a chemically active modification of nitrogen. It is suggested, provisionally, that the spectra are developed by the chemical union of this active nitrogen with the various metals and with iodine and acetylene. The orange glow obtained with nitrogen only would, on this view, be due to the transformation of the hypothetical active nitrogen into ordinary nitrogen. R. J. STRUTT.

Imperial College of Science and Technology,
January 30.

Singularities of Curves.

I HAVE not, at present, access to the books referred to by "T. J. I'A. B." in his letter of January 12; but he is altogether wrong in thinking that the singularity he mentions cannot be investigated by the methods explained in my "Geometry of Surfaces." An arbitrary line through the origin has sextactic contact thereat; but since the axis of x has 12-tactic contact at the origin, the latter cannot be an ordinary sextuple point, because no line through such a point can have a higher contact than septactic. The singularity is either a singular point of the sixth order or one of lower order with coincident branches passing through it, and it illustrates the necessity of drawing a distinction between ordinary multiple points and singular points. The trilinear equation of the curve can be obtained by eliminating t between $\beta=\alpha t^6$, $\alpha\gamma-\beta^2=\beta^2(t^3+t^4)$. The factor $\alpha\gamma-\beta^2$ suggests the existence of tacnodal or other branches of a similar character, and that the singularity might be transformed into a simpler one lying on a curve of lower degree than the sixteenth by using Cremona's transformation,

$$\frac{\alpha}{\alpha'\gamma'+\beta'^2}=\frac{\beta}{\beta'\gamma'}=\frac{\gamma}{\gamma'^2}$$

before applying the methods of chapter iv. of my book.

But it would have been foreign to the plan of my treatise to have introduced parametric methods when discussing singularities; moreover, the method of which the example is an illustration is only applicable to unicursal curves, whereas my own methods are independent of the deficiency. For example, the various singularities the point constituents of which are nine nodes could not be investigated by means of a unicursal curve without complicating the problem by introducing additional nodes isolated or in combination sufficient in number to reduce the deficiency to zero; and this might limit the generality of the investigation, for when the nodes exceed a certain number they are not arbitrarily situated, but lie on one or more dianodal curves. A. B. BASSET.

January 14.

MR. BASSET now admits that he has seen neither Zeuthen's two papers of 1876 nor Jordan's book of 1893, thus practically acknowledging the accuracy of my criticism—that the treatment of singular points in his "Geometry of Surfaces" is incomplete. With this admission from Mr. Basset the matter ends, so far as I am personally concerned.

But I must enter a protest against Mr. Basset's inference that the methods of Zeuthen and Jordan are only applicable to unicursal curves; since Mr. Basset has not read the work in question, his only reason for this statement is the fact that the example in my first letter happens to be a unicursal curve. This example was made up so as to provide a simple illustration of the general methods; but these methods hold good for curves of any deficiency.

It is absurd to suggest that parametric methods cannot be used for any algebraic curve; of course, the coordinates are expressed in the form of infinite series (convergent near a particular point of the curve) instead of terminated series. Mr. Basset's objection to using parametric methods would be quite justified if he had provided us with a satisfactory substitute; but he gives no *systematic* plan for resolving an assigned singularity, and this is the main object of the parametric method as used by earlier writers. T. J. I'A. B.

FRANCIS GALTON.

FEBRUARY 16, 1822—JANUARY 17, 1911.

THE death of Francis Galton marks, not only the removal of another link with the leaders of the great scientific movement of the nineteenth century—represented by Darwin, Kelvin, Huxley, Clerk-Maxwell, and Galton in this country—but something far more real to those who have been in touch with him up to the last, namely, the cessation of a source of inspiration and suggestion which did not flag even to the day of his death. The keynote to Francis Galton's influence over the science of the last fifty years lies in those words: suggestion and inspiration. He belonged to that small group of inquirers, who do not specialise, but by their wide sympathies and general knowledge demonstrate how science is a real unity, based on the application of a common logic and a common method to the observation and treatment of all phenomena. He broke down the barriers, which the specialist is too apt to erect round his particular field, and introduced novel processes and new ideas into many dark corners of our summary of natural phenomena.

The present writer remembers being asked some years ago to provide a list of Francis Galton's chief scientific achievements for use on a public occasion. It did not seem to him that a list of isolated contributions, such as the establishment of anthropometric laboratories, the introduction of the composite photograph, the transfusion experiments to test pangenesis, the meteorological charts and improved nomenclature, the practical realisation of the possibilities of finger-print identification, the demonstration of the hereditary transmission of the mental characters in man, the law of regression, the idea of stirps, or the foundation of the novel science of Eugenics, fully represented the nature of the man. What is the spirit of the contributions—large and small, almost two hundred in number—which Francis Galton made to the science of the last sixty years? [1] The unity of those contributions lay largely in the idea that exact quantitative methods could be applied, nay, rather must be applied, to many branches of science, which had been held beyond the field of either mathematical or physical treatment. In this manner his inspiration and suggestion tended to give physical and mathematical precision to a large number of outlying sciences, to meteorology, to anthropology, to genetics,

[1] His first contribution dates from 1849 and concerns a method of printing telegraphic messages at the receiving station.